

EXPERIMENTS

ON THE GENERATION OF

AIR FROM WATER;

TO WHICH ARE PREFIXED,

EXPERIMENTS

RELATING TO THE

DECOMPOSITION OF DEPHLOGISTICATED AND
INFLAMMABLE AIR.

From the Philosophical Transactions, vol. LXXXI. p. 213.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. AUREL. MED.
PARIS. HARLEM. CANTAB. AMERIC. ET PHILAD. SOCIUS.

DEUS NOBIS HÆC OTIA FECIT.

VIRGIL.

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1793.



THE
DEDICATION.

TO THE
MEMBERS OF THE LUNAR SOCIETY*

AT
BIRMINGHAM,

VIZ.

MATT. BOULTON, ESQ. JAMES WATT, ESQ. JAMES KEIR, ESQ.
DR. WITHERING, SAMUEL GALTON, JUN. ESQ. AND
THE REV. R. A. JOHNSON,
FELLOWS OF THE ROYAL SOCIETY.

My valued Friends,

THERE are few things that I more regret in consequence of my removal from Birmingham, than the loss of your society. It both encouraged and enlightened me; so that what I did there of a philosophical kind ought in justice to be attributed almost as much to

* The *Lunar Society* was so called because the members held their meetings every month, on the Monday that was nearest to the full moon, in order to have the benefit of its light in returning home. We had nothing to do with the *religious* or *political* principles of each other, we were united by a common love of *science*, which we thought sufficient to bring together persons of all distinctions, Christians, Jews, Mahometans, and Heathens, Monarchists, and Republicans.

you as to myself. From our cheerful meetings I never absented myself voluntarily, and from my pleasing recollection they will never be absent. Should the cause of our separation make it necessary, or expedient, for me to remove to a still greater distance from you, I shall only think the more, and with the more regret, of our past interviews.

It is now more than two years that I have been deprived of your society, and that I have been under the disagreeable necessity of intermitting my philosophical pursuits. With the assistance of my friends I have now resumed them, and it is with particular satisfaction that I dedicate the first fruits of my labours to you.

You are no strangers to the ostensible causes of those disgraceful riots which drove me from your neighbourhood, and you know my perfect innocence with respect to them. Had I really been what the populace, who demolished my library and apparatus, my house and every thing belonging to me, were taught to believe, viz. a fomenter of sedition, and an enemy to the peace and constitution of my country, symptoms of it must have been perceived in our frequent interviews. But you know that neither politics, nor religion, were ever the sub-
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jects of our conversations. Philosophy engrossed us wholly.

Politicians may think there are no objects of any consequence besides those which immediately interest *them*. But objects far superior to any of which they have an idea engaged our attention, and the discussion of them was accompanied with a satisfaction to which they are strangers. Happy would it be for the world if their pursuits were as tranquil, and their projects as innocent, and as friendly to the best interests of mankind, as ours. It is, however, a noble consideration, that the order and final happiness of the universe will not be interrupted by any thing that they can devise, or do, while there is one infinite mind superintending all things, and bringing good out of, and by means of, all the evil that they can contrive, or produce.

In the character of *a minister of religion* I never appeared with *you*, but as such (now that I am perhaps taking my final leave of you) permit me, though in religious persuasion differing from you all, to commit you to the protection and blessing of that Great Being, *whose we are, and whom*, I trust, *we all serve*, and who established that course of nature

which is the object of our common investigation; and when we meet, as I doubt not we shall, in another state, may the meeting be most happy.

With the greatest esteem and affection,
I am,

My valued Friends,

Sincerely yours,

J. PRIESTLEY.

Clapton, Nov. 16, 1793.

The P R E F A C E.

IN my *Appeal to the Public on the Subject of the Riots in Birmingham*, I said that, notwithstanding what had passed with respect to the repeated rejection of Mr. Cooper, recommended to the Royal Society by myself and several others, the most scientific members of it, I should not quarrel with the *institution*; but that if, on resuming my philosophical pursuits, any thing of importance should occur to me, I would endeavour to lay it before the public in the channel of their *Transactions*. This was my real intention at the time. But the Society, by rejecting another respectable candidate, who had the most ample testimonials in his favour, merely on account of his supposed political principles, has shown a fixed determination to reject any candidate whose political principles they do not approve; and this has made me conclude that any communications from *me* would be unacceptable to them. I therefore shall not

trouble the Society with them, but adopt this less eligible mode of publication.

Since, however, the friends of philosophy in this country *must* separate on the ground of *religion* and *politics*, things entirely foreign to our professed object, which is *natural science*, may the separation have no farther consequence than that of producing a generous emulation who shall most advance the common cause of science; the friends of *church and king*, as they affect exclusively to call themselves, or the friends of *liberty*, among whom, at all times, and in all circumstances, I shall be proud to rank myself. We are, it is true, but a small minority, but not deficient, I trust, in ability, activity, and energy; qualities which will always make men respected, though oppressed.

To the article that is now for the first time laid before the public, I prefix my last communication to the Royal Society, printed in their 81st Volume of *Philosophical Transactions*, p. 213, entitled, *Farther Experiments relating to the Decomposition of dephlogisticated and inflammable Air*, as I wish this publication to contain all my papers relating to philosophy not contained in the last edition of my philosophical works in three volumes octavo, or in my *History of Electricity*; and whatever I may hereafter publish of this kind I propose to give in the same form, that the different articles may in time make another volume.

The

The experiments of which an account is given in both these papers are unfavourable to the new system of chemistry, which excludes the doctrine of phlogiston, and though I have applied to the authors of that system in France, I have not yet been able to learn what they have to reply to what is advanced in my last communication to the Royal Society, which has now been two years before them; and in that I think I obviated every objection that I had heard to what I had before advanced on the subject. I perceive no difficulty in explaining the result of all their experiments on the received doctrine of phlogiston, but I do not see how they can explain mine, on the idea of there being no such thing. I hope, however, the discussion of the subject will proceed till the truth (which will, no doubt, finally prevail) be discovered. All difference of opinion, on this, as on every other subject, will only tend to give it the firmer establishment in the end. That I have no very strong attachment to any theories of my own, has appeared in all my past publications on philosophical subjects, and will farther appear in this.

I have already materials for another publication, but I defer the communication of them till I have pursued some of the experiments farther than I have hitherto done. Having objects enow of my own, I have always carefully refrained from putting my sickle into any other man's harvest. But if any
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person, seeing the track that I am in choose, to step before me, and anticipate me in any thing that I have in view, he is very welcome. The field is sufficiently ample to find employment for all the labourers, if we were ten times more numerous than we are.

As the time that I have to devote to these pursuits is very uncertain, and will probably not be long, I wish to make the most of it; and as the generous friends of science have enabled me to resume these pursuits, I wish to convince them that their confidence in me has not been misplaced. Least of all would I be deficient in gratitude to that Being, who is author of all good, and to whom, according to my *motto*, I ultimately ascribe the *means*, and the *leisure*, that I enjoy to prosecute these pleasing inquiries.

EXPERI-

EXPERIMENTS
RELATING TO THE
DECOMPOSITION
OF
DEPHLOGISTICATED AND INFLAMMABLE AIR.

From the Philosophical Transactions, Vol. lxxxi. p. 213.

THE doctrine of *phlogiston*, and that of the *decomposition of water*, have long engaged the attention of philosophical chemists, and experiments have sometimes seemed to favour one conclusion, and sometimes an opposite one. I have myself been very differently inclined at different times, as appears in my publications on the subject; and I am hardly sensible of a wish which way this important controversy, as it may be called, be decided, notwithstanding the part that I have taken in it. I cannot help thinking, however, that the experiments, an account of which I shall now lay before the Society, are decisive in favour of the composition of an acid from dephlogisticated and inflammable air; and, therefore, that the opinion of these two kinds of air necessarily composing *water* cannot be well founded. It is, indeed, sufficiently evident, that the same elements likewise compose *fixed air*, and therefore it is the

the less extraordinary that they should compose another acid.

The doctrine of phlogiston I would, however, observe, will not be affected by the most decisive proof of the composition of water from dephlogistified and inflammable air; since this would only prove, that phlogiston is one constituent part of water; which is an opinion that I have advanced, and mentioned on several occasions; and it is the less extraordinary, as water resembles metals in the remarkable property of being a pretty good conductor of electricity. What I shall now allege, however, will make it very doubtful, whether pure water be ever formed by the union of dephlogistified and inflammable air; and perhaps make it more probable, that water, as I have lately advanced, is only the *basis* of those kinds of air, as well as of every other.

It was objected to my former experiments on the decomposition of dephlogistified and inflammable air, by firing them together in a copper vessel, which always produced an acid liquor, that this acid came from the *phlogistified air* with which the dephlogistified air that I made use of was necessarily more or less diluted; or from that which I could not wholly exclude, as a part of atmospherical air, when I exhausted the copper vessel by means of an air-pump.

To obviate this objection, I then observed, that I not only constantly found that the more phlogistified
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cated air was contained in the two other kinds of air (mixed in the proportion of two measures of inflammable air to one of dephlogistified) the less acid I got; but that, when I purposely mixed any given quantity of phlogistified air with them, it appeared not to have been at all affected by the process, but remained the very same, in quantity and quality, as before. Still, however, because Mr. Cavendish, though in a very different process, had found nitrous acid to result from the decomposition of phlogistified and dephlogistified air; and because M. Lavoisier and his friends had found nothing but pure water after the slow burning of dephlogistified and inflammable air; it was maintained by the favourers of their system, that the *water* only in the liquor which I procured came from the union of the two kinds of air, and the *acid* from the *phlogistified air* which I had not been able to exclude.

But let any person only consider the very small quantity of nitrous acid which was procured by Mr. Cavendish from the certain decomposition of 3194 grain measures of atmospherical air, amounting to more than $6\frac{1}{2}$ ounce measures in one case, and of 2710 grain measures, amounting to $5\frac{1}{2}$ ounce measures in another case (Phil. Transactions, Vol. LXXVIII. p. 264. 268.), three-fourths of which was phlogistified air; and the vastly greater quantity which I procured (Ibid. p. 324.), when it could not be proved, that a particle of phlogistified

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cated air was decomposed, and think whether it was at all probable, that the acid came from this kind of air, and not from the union of the dephlogisticated and inflammable air, which evidently disappeared in very great quantities. This circumstance alone might have satisfied those who interest themselves in this question; but it does not seem to have been attended to.

I have now, however, effectually removed the objection above mentioned, by intirely excluding all phlogisticated air from the process; the dephlogisticated air which I at present use being so pure, that it contains no sensible quantity of phlogisticated air. I also make use of no air-pump, but first fill the copper vessel with water, and then displace it by the mixture of the two kinds of air; yet, in these circumstances, in which all phlogisticated air is excluded, I procure even a stronger acid than before.

The paper that I send along with this article contains the dry residuum of the turbid green liquor, produced by a single explosion of a mixture of two parts inflammable and something more than one part of dephlogisticated air, in a copper vessel which holds thirty-seven ounces of water; and a little more must have remained in the vessel, which I could not get out by draining or shaking it. It is most evident, therefore, that the acid necessary to dissolve so much copper must have come from the union of the dephlogisticated and inflammable air,

because there was nothing else in the vessel. The inflammable air was procured from iron by means of steam.

This very pure dephlogisticated air I first imagined could only be got by the process in which I observed (Experiments on Air, Vol. II. p. 170.) that I once before procured it, though I then supposed the extraordinary result to be accidental; because in other circumstances I have sometimes had it very pure when I could not succeed in a second attempt of the same kind. It was by heating the yellow product of the solution of mercury in spirit of nitre, without suffering the red precipitate, into which it is converted by heat, to come into contact with the external air, from which I thought it probable that it might attract some phlogiston. Afterwards, however, I found that this circumstance makes no difference whatever; and that the air so procured *appearing* to be purer, arose from the greater purity of the *nitrous air* which I made use of as a test, and which I got from mercury, and not from copper, the nitrous air from which I find to be much less pure. For trying the dephlogisticated air yielded by some red precipitate which had been prepared many months by the nitrous air from mercury, it appeared to be as pure as that which was procured in the manner above described.

That the dephlogisticated air which I now made use of was sufficiently pure for my purpose, appeared from mixing one measure of it with two of
nitrous

nitrous air, when the whole quantity was reduced to less than four hundredth parts of one measure; so that it is probable that, by a more accurate proportion of the two kinds of air, and greater address in mixing them, they might have almost intirely disappeared. There is besides some reason to think, from the great variety in nitrous air, that the greater part of this very small residuum comes from the nitrous air, and not from the dephlogistified.

It will be said, how is it possible to reconcile the result of this experiment with that of M. Lavoisier and his friends? which I was by no means disposed to question after the publication of the *Extract from the Register of the Academy of Sciences for August 28, 1790*, in the seventh volume of the *Annales de Chimie*, in which a distinct account is given of a large quantity of very pure water procured from the slow combustion of the two kinds of air above mentioned: for before this it was acknowledged, that some little acid was always found in the water so procured.

But my late experiments, besides ascertaining the fact of the production of nitrous acid from the decomposition of dephlogistified and inflammable air, throw some farther light on the subject, and may in some measure explain their result; for I am now able to procure, in my own process, either nitrous acid, or pure water, from the same materials.

I con-

I constantly observe, that if there be a surplus of dephlogistified air, the result of the explosion is always the acid liquor; but that if there be a surplus of inflammable air, the result is simply water. That phlogistified air is not in all cases affected by this process, I completely ascertained, by admitting a little common air into that mixture of the two kinds of air which always produced water, and finding nothing but water in the result.

I find, however, that, agreeably to the experiments of Mr. Cavendish, phlogistified air is decomposed in this process, when there is not enough of inflammable air to saturate the dephlogistified air; though when there is a redundancy of inflammable air, there is even a production of phlogistified air. Putting 0.5 oz. m. of phlogistified air to a mixture of two ounce measures of inflammable air and 1.5 oz. m. of dephlogistified air, the whole was reduced by explosion to 1.05 oz. m. of the standard of 1.1, with two measures of dephlogistified air, which appears by computation to contain no more than 0.388 oz. m. of phlogistified air; so that 0.112 oz. m. had been decomposed in the process. When there is a sufficient quantity of inflammable air, the phlogistified air always remains unaffected in this process, as appears by mixing any quantity of it with the two kinds of air to be exploded, and finding the very same quantity, as I have repeatedly done, in the residuum.

That when there was a sufficiency of inflammable

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air

air for the purpose, phlogisticated air is even *produced* in this process, was evident from my never being able to diminish any quantity of dephlogisticated air by inflammable air so far as by good nitrous air, and the residuum always containing phlogisticated air. Having exploded two measures of inflammable air with one of dephlogisticated air, which by a mixture of two measures of nitrous air was reduced to 0.04, there was a residuum of 0.1, of the standard of 1.3, which appears by computation to contain 0.0767 oz. m. of phlogisticated air.

The reason why, in my former experiments, I always procured more or less acid, must have been that, without any intention, or suspecting that any thing depended upon it, I must have had some surplus of dephlogisticated air. M. LAVOISIER I also perceive to have taken it for granted, as I did, that after either of our processes, any surplus of either of the two kinds of air would only have remained unfaturated, and have been found unchanged in the residuum.

I claim no merit whatever in this observation. It was in consequence of accidentally finding pure water in what I then imagined to be the same circumstances in which I had always before found acid, and which surprized me not a little at the time, that I was led to vary the proportions of the two kinds of air, till at length I succeeded in ascertaining the circumstances on which this remarkable difference
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in the result depends; but I am by no means able to assign any reason for this difference.

In this state of my experiments I concluded, that *nitrous acid*, though consisting of the same elements with pure water, contains a greater proportion of dephlogisticated air; and in the last edition of my *Observations on Air*, vol. III. p. 543, I observed, that, "substances, possessed of very different properties, may be composed of the same elements, in different proportions, and different modes of combination. It cannot therefore be said to be absolutely impossible, but that water may be composed of these elements," viz. dephlogisticated and inflammable air.

When I first prepared an account of my late experiments for the Royal Society, I entertained this idea; but now I consider it as at least uncertain, because when I mix the two kinds of air in such proportions as to produce *water*, I find in the residuum much more *phlogisticated air* than I do when *acid* is produced, which affords a suspicion that, in this case, *the principle of acidity* goes wholly into the phlogisticated air, which, as my former experiments shew, actually contains it, though it is not easy to ascertain in what proportion.

Having exploded three ounce measures of a mixture of something more than two parts inflammable air, and one of dephlogisticated, and another equal quantity in which the inflammable air bore a less

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proportion to the dephlogisticated (the former of which I knew would yield water, and the latter acid) I found the residuum of the former to be 0.57 oz. m. not affected by nitrous air, and weakly inflammable; and in order to find how much phlogisticated air it contained, I mixed different proportions of phlogisticated and inflammable air, and concluded, from the manner of firing *them*, and this *residuum*, that it could not consist of less than one-third of phlogisticated air, *viz.* 0.19 oz. m. But the residuum of the mixture which would have produced acid was 0.62 oz. m. of the standard of 1.0, which I find by computation to contain not more than 0.062 oz. m. of phlogisticated air. I repeated this experiment very many times, and never failed to have a similar result; so that it is very possible that the *pure water* we find may be nothing more than the basis of the two kinds of air; and the principle of acidity in the dephlogisticated air, and the phlogiston in the inflammable air, may combine to form a superfluous acid in the one case, and the phlogisticated air in the other.

This supposition is strengthened by finding that whether the produce be acid or pure water, the two kinds of air unite in nearly the same proportions. But since water has an affinity to almost every substance in nature, and a peculiarly strong one to the acid and alkaline principles, it may be impossible that it should be wholly free from them; and if they

they be in proper proportions to saturate one another, and in the same quantities, their presence may never appear.

As the reason why, in my former experiments, I always produced an acid liquor, and never pure water, was my using too great a proportion of dephlogistigated air; so the reason why M. LAVOISIER and his friends generally produced but little acid, and at last none at all, must have been, that the *slow combustion* which they made use of gave the principle of acidity in the dephlogistigated air, and the phlogiston in the inflammable air, a better opportunity of escaping, and forming the phlogistigated air in their residuum, of which they have not published any satisfactory account*; and it is probable, that the weight of these elements compared with that of the water which forms the basis of the two kinds of air, may be very small. That excellent philosopher M. DE LUC supposes that they have even no weight at all.

M. LAVOISIER himself, I observe, lays particular stress (p. 262) on the *slowness* of the combustion, as if he suspected it to be necessary to his result. This circumstance may also account for my want of

* Since this was written, Mess. FOURCROY, VAUQUELIN, and SEGUIN, have published a very particular account of their experiment; from which it appears, that, after the combustion of the two kinds of air, there was a pretty large residuum of phlogistigated air, more than was contained in the airs before combustion. See *Annales de Chimie*, for April 1791, p. 35.

success in the attempts that I made to repeat his experiment: for whenever I made a stream of inflammable air to burn in a vessel of dephlogisticated air (which I contrived to do by means of a less expensive, but I own a less accurate, apparatus than his), I always got some acid, though less than in my own process; but I made a larger and stronger flame than I imagine M. LAVOISIER chose to produce.

In the course of these experiments I found, that when the inflammable air I made use of was from turnings of *cast iron*, there was always a considerable quantity of *fixed air* in the residuum, not less than one tenth of a measure, after the explosion of two measures of inflammable air and one of dephlogisticated; whereas there was either no fixed air at all, or the slightest appearance of it imaginable, when I made use of inflammable air from *malleable iron*, extracted either by means of steam or acids.

The principal of these experiments, as well as those in my former papers on this subject, will be found to confirm the similar ones of Mr. CAVENDISH; but they prove the source of the *acid* in the results not to be what he imagined, *viz. phlogisticated air*, but the union of the deplogisticated and inflammable air; and they also make it at least doubtful, whether these two kinds of air compose *pure water*.

EXPERIMENTS

ON THE

GENERATION OF AIR FROM WATER.

THERE are two problems, of considerable importance, relating to the doctrine of air, to which I have had an eye ever since I began my experiments on the subject; but concerning which I have been far from being able to get all the light that I have wished. One is the constitutional difference between the *vapour* of water and other fluids, and *air*, or a permanently elastic substance, not condensable by cold; and the other is the *origin of our atmosphere*. But the experiments I have lately made seem to give us some insight into both.

That *heat*, in a latent or fixed state, is essential to the constitution of all kinds of air, seems to be generally taken for granted; and several of my former experiments demonstrate it with respect to inflammable air; nothing but heat being necessary to convert spirit of wine, oil, and other inflammable sub-

stances, liquid or solid, into permanent inflammable air. Alkaline air is also converted into inflammable air by the same process, and by the electric spark, which produces this effect merely by communicating heat.

Light also is necessary in some processes to the formation of dephlogisticated air, for without this growing vegetables cannot produce it, as I discovered first with respect to the green vegetable substance to which botanists have not yet given any name, and immediately afterwards with respect to larger plants*. Light is also necessary to extract dephlo-

* As some persons imagine there is an interference in Dr. Ingenhourz's claims to discovery and mine in this respect, I shall subjoin an extract of a letter I wrote to him six years ago, when a young physician on his travels desired of me letters of introduction to my philosophical friends on the continent, since it will tend to set the matter in a proper light, and shew that there is no ground of interference between us at all,

DEAR SIR,

Birmingham, Nov. 21, 1787.

I thank you for the French edition of your *Experiments on Vegetables*, which I received some time ago; but am sorry to see in the *preface*, something that looks as if you, or your friends, thought I wished to detract from your merit, which is very far from my disposition.

I do not indeed distinctly see what ground there is for any interference between us. That plants restore vitiated air I discovered in a very early period. I afterwards found that the air in which they were confined was sometimes even better than atmospheric air, and that the *green matter* (which I at first, and several of my friends always, thought to be a vegetable) produced pure air by means of *light*; and immediately after the publication

dephlogisticated air from water, impregnated with dephlogisticated marine acid air. The experiments on

cation of this fact, and before I saw your book, I had found that other whole plants did the same.

All the time that I was employed in making these experiments I wrote to my friends about them, particularly Mr. Magellan, and desired him to communicate my observations to you, as well as to others, but I believe you did not hear of them; so that what you did with *leaves* was altogether independent of what I was doing with *whole plants*; but the same summer, and the same sun, operated for us both; and you certainly published before me.

This appears to me to be the true state of the case, and surely it leaves no room for the suspicion of any thing unfair, or unfriendly. But whatever your friends may say, I have no thoughts of troubling the public with any vindication. I value you, and your friendship, too much to wish to have any altercation on the subject. Indeed, there is nothing to contend about. If, on any future occasion, you will do me the justice to give this statement of the matter, I shall be happy. If not, I shall not complain.

I hope you have received my last volume of experiments. I am now busy with others, and have lately proved, that a very great part of fixed air is water, and, what is perhaps of more consequence, I find that, in many cases, the decomposition of pure and inflammable air produces little water, but acid; though no acid was employed in procuring either of the kinds of air so decomposed. What the acid is I have not yet ascertained, but hope to do it soon. I shall always be happy to hear from you, and am,

Dear Sir,

Your's sincerely,

J. PRIESTLEY.

This,

on taking the electric spark in water by Messrs. Die-
man and Van Troostwyck, who thought they had
by this means decomposed it, and converted it into
dephlogisticated and inflammable air, led me to try
the effect of the sun-beams upon it; in consequence
of which I never failed to produce air, but not in
quantity sufficient to ascertain its nature. I have
lately repeated this experiment with the same result,
and have made several others which do not confirm
the hypothesis of those philosophers, but lead us a
step farther in this curious and important investi-
gation, especially as they shew the influence that heat
has in this business.

That water which has been distilled, or boiled,
has an eager attraction for air, is a fact universally
known. It will imbibe any kind of air whatever;
and, as I have found, will give out, by heat, the same
air with which it has been impregnated. So obsti-
nately does water retain air, that M. De Luc was
never able completely to free any quantity of water

This, I think, will shew that I was not very anxious about the
merit of this discovery. The original observation, that plants re-
store vitiated air, and the subsequent one, that *green vegetable*
matter yielded dephlogisticated air by means of light, were both
accidental; and surely there was no great sagacity required to
try the effect of *certain* plants, when I had ascertained the fact
with respect to one of a *doubtful* nature. That *leaves* had this
power, I own, I had no suspicion. But the merit of all philo-
sophical discoveries is in my opinion greatly overrated.

from it, and for the same reason attempts to make barometers of it have never succeeded.

The circumstance that led me to give particular attention to this subject of late was, that, in distilling a large quantity of water, I found that air was produced not only at the first boiling, and for half an hour afterwards (which I had been used to deem sufficient to expel all the air that water contained) but uniformly, to the very last; which shewed that the air so procured was not any that had been previously contained in the water, and had been expelled by heat, but what was actually formed in the process.

In the first experiments that I made to collect this air, the steam was made to issue from the end of a glass or copper tube; and when it was received in a glass recipient containing water, every bubble of steam collapsed into a small bubble of permanent air. A much greater proportion of it, however, was permanent in a degree of heat much below that of boiling water; as when I could very well bear to handle the glass vessel in which it was contained, and indeed when it was but sensibly warm. Also, after this newly-generated air was transferred to cold water, saturated with air, it would continue some time without any sensible diminution; but in the course of a day or night some times so much as one fifth of it (though generally a much less proportion) would be absorbed. But the remainder was as permanent air as that of the atmosphere;
and

and when it was examined by the test of nitrous air, was generally much purer than common air, the standard of it, with an equal quantity of nitrous air, being 0.9, 0.85, or even 0.8, instead of 1.0, or 1.1, which is the usual standard of atmospherical air after being agitated on its mixture with nitrous air. I have found it to have the same degree of purity whether the water was boiled in a glass vessel, in one of copper, or of tinned iron, though I have found it much more impure when boiled in a copper vessel, and the reason of this variety I do not yet understand. This air was not a mixture of dephlogisticated and inflammable air; for then it would have exploded on applying a candle to it; but a candle burned in it exactly as in atmospherical air, only something better.

Being willing to ascertain whether the air I procured came from the water that had been converted into steam, or, in part at least, from that with which the steam came into contact when it was condensed, I made it to issue from under a quantity of mercury; and, to my great surprise, I found the quantity of air greatly diminished, even when the mercury became as hot (which it soon did) as the steam itself, and consequently the steam passed through it uncondensed. When, with the same view, I caused the steam to be condensed in water recently boiled, and warm, I found no diminution of the quantity of air.

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I am unable to account for this diminution of the quantity of air after the steam had passed through mercury. To ascertain whether the pressure of such a column of mercury had any influence (though to avoid this I only just covered with it the end of the tube through which the steam issued, without finding any sensible difference) I plunged the tube from which the steam issued at least one foot below the surface of a quantity of water in which it was received, which was equivalent to one inch of mercury, and then also I found a considerable diminution of the quantity of air; but suspecting that it could not be owing to mere *pressure*, I placed the tube through which the steam passed horizontally, only just covering it with water, and the result of this appeared more extraordinary still; but I repeated the experiment too often to be mistaken with respect to the fact.

If the steam be suffered to issue in a basin of water, with only the end of the tube by which it is conveyed from the boiler, covered with water, more or less air is always formed, at a medium between one twentieth and one thirtieth of the bulk of the water; but if the pipe be covered with water, so that the steam shall be condensed before it reaches the orifice, little or no air is produced; and the result is the same whether the tube be of copper or of glass, provided it do not exceed about one sixth of an inch in diameter. If it be about half an inch wide,
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air will always be formed, though the steam be condensed within it.

In general, I have found more air produced when the steam has been made to pass through a red-hot well glazed earthen, or copper tube, filled with bits of earthen ware, in order to expose it to as much heated surface as possible; but still if the tube of conveyance was narrow, and covered with water, so that the steam was condensed within it, no air, or next to none, has been procured. The following was the result of one of the experiments of this kind.

From 25 ounces of water distilled through a red-hot copper tube, with the tube by which the steam was conveyed not covered with water, I got two ounce-measures of air, the first portion of which was of the standard of 1.0, and the second of 1.1. But from 19 ounces of water distilled in the same circumstances, but with the tube by which it was conveyed covered with water, I got only 0.15 of an ounce-measure, of the standard of 1.2. I recite the differences in the purity of the air, as I noted them at the time, without supposing that those differences arose from any circumstances that I have mentioned. For I have found this to vary very much in what appeared to me to be the same circumstances. This deserves a farther investigation.

Having procured very different quantities of air from the same quantity of water, and having observed the

the very great difference above mentioned arising from the steam issuing in the basin of water, I imagined it might be necessary to the formation of the air, that the bubbles of steam should be completely surrounded with water at the moment of their condensation, and that the greater number of those bubbles I could make the more air I should procure. With this view I fixed to the end of the tubes from which the steam issued small pieces of tin with many perforations; and I thought that this was, upon the whole, the surest method of procuring the largest quantity of air. But when the water in which the steam was condensed was very hot, and consequently it has issued in large bubbles, which have branched out into many parts before they were condensed, so as to produce a great number of separate bubbles after condensation, I have generally procured as much air as in any circumstances whatever.

In all the preceding processes, though the air was procured from water that had been boiled ever so long, or ever so often, and it was received in water that had been boiled, or by other methods deprived as far as possible of all air, it might be said that it was still possible that the air I procured might be only atmospherical air, intimately united with the water, and not properly a fresh production of it; especially as the purity of it was so very nearly that of the atmosphere. In order to ascertain whether this was the case, or not, I contrived to use the same water repeatedly, confined by mercury, than which I do
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not know that any process can be more unexceptionable.

Filling long glass tubes, closed at one end, partly with water, and partly with mercury, the open ends being immersed in basons of mercury, I began with exposing a part of the tube above the mercury to heat, so as to convert the water contiguous to that place into steam, the water having been previously made as free from air as possible. Still, however, I observed that every bubble of steam collapsed into a bubble of air, which ascended to the top of the tube; and many of them being there collected, formed a larger bubble, which continued permanent air, when the fire was withdrawn, and the tube was cold. In the course of a day or two this air would not fail to be absorbed by the water in the tube; but when I inverted the tube, and threw out that air, by putting in more mercury, more air was always generated by repeating the process.

To procure a larger quantity of air, I varied the process, by making the tube containing the water and mercury lean over the fire, so that about twelve or eighteen inches of the upper part of it was kept many hours together in the state of a vapour; and I constantly observed that whenever the fire was withdrawn, the water never completely filled the tube, but a quantity of air remained at the top. This I threw out as before, and repeating the process with the same water, day after day, and week after week, I always found that the air was produced just as at the first, and

and that the longer the tubes had been exposed to the heat at any particular time, the more air I found in them.

Still, however, it might be said, that, as I turned the tubes upside down in order to let out the newly-generated air, so that the water in the tube was for a moment exposed to the common atmosphere, though this was done while the water was warm, and consequently not disposed to imbibe air, it was not absolutely impossible but that it might do it, I therefore provided a long trough of mercury, and plunging the tubes into it, let out the air under the mercury, so that the water never came into any contact with the air of the atmosphere. But I always found that when the tubes were again brought to the fire, and the water converted into steam, air was produced as before; and I did not cease to repeat this process, till I was thoroughly satisfied that the repetition would answer no purpose. I therefore could not help concluding that, on whatever principle the effect was produced, the whole of any quantity of water is convertible into air by means of heat, or by heat and light jointly. For the glass tubes in this set of experiments being transparent, light might have some influence in the business.

In order to ascertain whether light had any influence in these processes, or not, in boiling the water I used not only glass retorts, and glass tubes, but vessels and tubes of copper, and received the steam in vessels perfectly opaque, so that no light whatever

had the least access to the water, or the steam, in any part of the process; and yet air was produced as when it had. In like manner I coated the glass tubes with tin foil, and tinned iron, but still by exposing them to heat the effect was always the same, in kind and degree, that it had been before they were so coated. I therefore concluded that *heat*, and not *light*, was the agent in this conversion of water into air.

There is another process in which air is produced from water without heat, except such as we have no means of excluding. In order to free a quantity of water from the air it contains with as little trouble as possible, I had been long used to put a pretty large quantity into a glass vessel, of a globular form, furnished with a stem of the length of a barometer tube, then after filling the bulb with water, and putting in mercury, till the tube was full, inverted it, placing the end of the tube in a basin of mercury; when, the pressure of the atmosphere being removed, the air contained in the water is discharged, as it would be by means of an air-pump. Whenever I have done this I have observed that after throwing out the air that I got, and replacing the vessel as at first, I still never failed to get more air, though less than at the first. This air, however, I used to consider as that which had been *extracted* from the water. But the preceding experiments led me to consider it as *produced* from the water, either by means of heat, or of light, and a greater degree of attention to this process

process has confirmed me in this opinion. I find, however, that it is not *light*, but probably *heat* only, that produces this effect, in this process as well as in the former. I have one of these vessels which has had water in it more than a year, and yet it continues to give air just as it did after that which it naturally contained was expelled. If I examine it every day, I find fresh air in it, so that I am persuaded there will be no termination of the production.

It is easily known when any quantity of water is purged of the air it naturally contains in this process, by its rattling on agitation, exactly as in a water hammer: but even after this it does not fail to yield air. At one time I found the column of mercury in a vessel of this kind stand at $31 \frac{1}{4}$ inches, when that in the barometer was only 30; and it was not without a good deal of agitation that the water could be separated from the glass, and permit the mercury to fall lower. This is a common case with mercury in a dry glass tube, but I never observed it more than this once with water. It was a proof, however, that at that time it contained no air, and yet when a vacuum was made by the subsiding of the water, air began to be produced as before.

Agitation greatly promotes the discharge of air from water in these circumstances, and two things are particularly remarkable with respect to it; one is, that the air issues not only from the parts contiguous to the glass, but from every part of the water in the

interior of the vessel; and the other is, that the full effect of the agitation does not appear immediately, but about half a minute after, when, at the beginning of the process, the water will be almost opaque with the multitude of small bubbles formed through its whole mass.

The quality of the air procured in this manner varies much, and I have not yet observed the rule by which it varies. At one time I found the air procured at first inferior to that which was produced the second time that it was collected, and the third successive portion was purer still; but this has not, I think, always been the case.

After giving much attention to the subject, I see no reason to think that *light* has any influence in the generation of air in this process any more than in the former. I have kept one of these vessels with water in it a long time, covered with tin foil, and so well purged of air that it rattled like a water hammer, and yet whenever it is inverted, either after waiting some time, or after agitation, a pretty large bubble of air is found in it.

Whatever be the agent in this production of air from water, according to appearance, the whole of any quantity of water is convertible into air by means of heat, or some principle of repulsion acquired in the act of converting it into vapour. It should seem, therefore, that the whole of the atmosphere may have been originally formed from water by means of heat; and this would at first be rapid, without any

sensible heat, there being no pressure of a previously existing atmosphere to prevent it, and afterwards volcanos might assist the process. At present air may be produced by the action of the sun on the vapour of the upper region of the atmosphere.

Since the atmosphere consists of both dephlogisticated and phlogisticated air, or something equivalent to a mixture of the two, it is evident from the preceding experiments that (unless we can suppose both the *phlogistic*, and if I may so say, the *antiphlogistic* principle, to pass through the glass together with heat) water must contain both these elementary ingredients, which is an idea which neither myself nor the French chymists had formed of it; since, according to them, it consists of dephlogisticated and inflammable air; and phlogisticated air, (or, as they call it, *azote*,) is a simple element, not contained in water, and I and other chymists had considered water as a simple elementary substance, though easily combining with almost every other substance in nature.

Still, however, what I have before advanced concerning water, of whatever elements it may consist, viz. that it is the proper basis of every kind of air, may be, and probably is, strictly true. It may even be all that has, or can be ascertained by, *weight* in most of them; so that when they are decomposed, nothing else may be found. Had it appeared, as I was once inclined to think it would, that *light* was necessary to the production of air from water without heat, I should have concluded that the pure air yielded by
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vegetables was occasioned by the action of light upon the watery effluvia issuing from them; but as in this case air is produced without light, and plants do not produce any air in the night, the principle on which these curious processes is performed must be different.

It appears from these experiments that water is resolvable into dephlogisticated and phlogisticated air. To complete the analysis we should reduce those two kinds of air to water. But as *heat* is necessary to the decomposition of water, the absence of heat, or *cold*, and such a degree of it as we cannot produce, may be necessary to the composition of it from those elements.

I cannot conclude this paper without observing that the advances we are continually making in the analysis of natural substances into the *elements* of which they consist, bring us but one step nearer to their constitutional differences; since as much depends upon the *mode of arrangement*, concerning which we know nothing at all, as upon the elements themselves. For things the most different in their properties appear to consist of the very same elements. Thus the nitrous acid, nitrous air, fixed air, phlogisticated air, alkaline air, and probably all the other kinds of air with which we are acquainted, except the dephlogisticated, and inflammable, are all composed of dephlogisticated air and phlogiston. For that phlogisticated air is not an elementary substance, but composed of the two things above mentioned

tioned, sufficiently appears from my former experiments, and others that might be alleged. By what means we can ever come at the knowledge of the internal arrangement of the elementary parts of natural substances, I have not the least idea. Notwithstanding this, the success with which our inquiries have hitherto been crowned should encourage us to despair of nothing. Nature exhibits an infinitely large field, but our powers of investigation seem also to increase without limits.

FINIS.